comment; and they will have served their purpose if they direct attention to the part played in the phenomena of terrestrial magnetism by moving conductors. It will be noticed that they leave untouched the mysterious and important connection between sun spots and magnetic disturbances.
B. STEWART

P.S.—Since writing the above, Prof. Sir W. Thomson has informed me that Faraday tried to detect induction currents by tides in the Thames, but found no positive result. In an article in the *Philosophical Magazine*, Dec. 1851, Prof. Sir W. Thomson quotes this idea of Faraday, and makes a proposal to test it from tides in the Channel. He also discusses the part which may be played in the phenomena of terrestrial magnetism by moving conductors.

BRITISH RAINFALL

MR. G. J. SYMONS (62, Camden Square, N.W., December 22, 1869) sends us the following list of localities whence observations are "urgently required;" we think he will have many volunteers:—Cornwall: Falmouth, Jacobstow. Devon: Hatherleigh, Hartland, Exmoor. Dorset: Bere Regis. Oxford: Thame. Surrey: Redhill. Suffolk: Halesworth, Mildenhall. Lincoln: Kirton. Nottingham: Mansfield. Warwick: Stratford-upon-Avon. Shropshire: Bishop's Castle. Yorkshire: Milford Junction, Pateley Bridge, Kettlewell, Askrigg, Driffield, Bridlington, on the North York Moors. Lancashire: Broughton. Northumberland: Haltwhistle. Cumberland: Kirkoswald. Westmoreland: Ravenstonedale. Isle of Man: any part of the Island. Pembroke: Tenby, Fishguard. Cardigan: Aberaeron, Brecknock, Llanwrtyd. Radnor: Builth, Knighton. Montgomery: Montgomery, Llanfyllin. Merioneth: Barmouth, Harlech. Carnarvon: Pentrevoclas. Wigtown: Northern part of. Kirkcudbright: Western part of. Peebles: Peebles, Biggar. Ayr: Muirkirk. Argyle: Mull of Cantire, Ballimore, Glencoe, near Ben Cruachan. Perth: North-west part of. Forfar: Western part of. Inverness: along the Caledonian Canal, and in Lochaber. Aberdeen: North-west part of. Nairn: any part of. Banff: Southern part of. Ross: any place inland, or in Lewis or Harris. Sutherland: any place inland. Caithness: any place inland. Ireland: except from the vicinity of Belfast, Dublin, Londonderry, and Waterford, where there are many observers, returns are required from nearly all parts of the country.

THE LATE PROFESSOR MICHAEL SARS, OF CHRISTIANIA

THIS eminent zoologist died on the 22nd of October last; and his loss will be much felt by all naturalists who have benefited, as I have done, by his long, laborious, and conscientious investigation of the invertebrate fauna of the Norwegian seas.

He was born on the 30th of August, 1805, at Bergen, where his father was a shipowner. After finishing his academical studies at Christiania, and evincing at an early age his predilection for natural science, he entered into priest's orders, and in 1830 became pastor at Kinn, in the diocese of Bergen. Ten years afterwards he had charge of the parish of Manger in the same diocese. As both these parishes were on the sea-coast, Sars had constant opportunities of pursuing his zoological researches. In 1829 he published his first essay, entitled "Bidrag til Söedyrenes Natur-historie," and in 1846 the first part of his celebrated work "Fauna littoralis Norvegiæ." In 1854 he was appointed Professor Extraordinarius of Zoology at the University of Christiania, a position which he filled up to the time of his lamented death with great honour to his country, and to the satisfaction of the whole world of science. His celebrity as a zoologist, as well as a palæontologist, was fully recognised by all naturalists and geologists, and he was elected a member of several foreign scientific societies. Our own distinguished countryman, the late Edward Forbes, individually showed his appreciation of Sars's labours in eloquent pages (66 and 67) of his own posthumous work, "The Natural History of the European Seas," when he said, "More complete or more valuable zoological researches than those of Sars have rarely been contributed to the science of Natural History, and the success with which he has prosecuted investigations claiming not only a high systematic value, but also a deep physiological import, is a wonderful evidence of the abundance of intellectual resources which genius can develop, however secluded and wherever its lot be cast;" and he added that the name of this Norwegian priest, "who reaped reputation when seeking no more than knowledge familiar to every naturalist in Europe and America, in Asia, and at the Antipodes—for there are great naturalists settled far in the south, and many in the far east—is a sufficient proof that able work brings the rewards of applause and veneration, even when they be unasked for." By the observations of Sars on the development of the Medusæ he greatly advanced our knowledge of that remarkable physiological phenomenon known as the alternation of generations, which Chamisso had first indicated in the Salpæ. His last publication, "Mémoire pour servir à la connaissance des Crinoïdes vivants," caused especial interest, by showing that a race of animals, supposed to be extinct for a period so long as only to be measured by the duration of several past geological epochs, occurred in a living state in the abysses of the Norwegian seas. This discovery mainly induced the recent exploration of our own seas at great depths, which has produced such wonderful results; and the living Crinoid, or "stone-lily" (*Rhizocrinus Lofotensis*), has now been ascertained to inhabit many parts of the Atlantic from the Loffoden Isles to the Gulf of Mexico. The published works of Sars are seventy-four, and they are not less sound and valuable than numerous. One of his sons, Dr. George Ossian Sars, inherits the zoological inclinations and talent of the late Professor, and is second to none in the knowledge of the Sessile-eyed Crustacea.

It is exceedingly to be regretted that, in spite of the most rigid economy, the large family of Professor Sars is left in very impoverished circumstances, six of his children being wholly unprovided for. May I hope that naturalists and lovers of science will assist me in making a subscription for the temporary relief of this distressed family, and that they will by such tribute to his memory express their admiration of his career and services? I shall be very glad to receive any contributions.

J. GWYN JEFFREYS

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

Cuckows' Eggs

I AM very grateful to Mr. Sterland for asking for further information "on some points of difficulty" in my recent paper on Cuckows' Eggs, because it shows me where I have failed in making myself plainly understood. In endeavouring to reply so far as lies in me to his questions, I will take them in order.

1. "Are they [Cuckows' Eggs] so variable as some assert?"

Mr. Sterland supports the doubt here indicated by the statement of "one of the most eminent and experienced of living oologists;" but who this oologist may be he leaves to be guessed, and I venture with all respect to remark, that quoting an anonymous authority in natural history is quoting no authority at all. I am therefore not willing to bring such experience as I myself have had into conflict with that of this eminent but nameless person. Still, as Mr. Sterland is not satisfied with the opinion on this

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point of the German authors* cited by Dr. Baldamus, I will here adduce the following passages:—

"Quant au genre Coucou (*Cuculus*) et notamment à l'espèce type du genre, notre Coucou Chanteur (*C. canorus*), on sait quelle étonnante diversité offre la coloration de son œuf, toujours de forme ovée, diversité telle que nous nous abstiendrons d'en aborder la description détaillée."—DES MURS, *Traité Général d'Oologie Ornithologique*, &c. Paris: 1860, p. 219.

"Ces œufs sont très-petits relativement à la taille de l'oiseau, et varient beaucoup pour la couleur. Ils sont ou cendrés, ou roussâtres, ou verdâtres, ou bleuâtres avec des taches petites et grandes, rares ou nombreuses, d'un cendré foncé, vineuses, olivâtres ou brunes, avec quelques points et parfois des traits déliés noirâtres. Nous en possédons deux du blanc le plus pur, et un autre d'une seule teinte bleu-verdâtre, pris dans un nid de Stapazin."—DEGLAND et GERBE, *Ornithologie Européenne*, &c. Paris: 1867, vol. i. p. 163.

I produce this testimony as to *facts* with the greater confidence, because the *opinions* of the witnesses differ from my own, and not one of them, so far as I can gather from their works, was acquainted with Dr. Baldamus's essay.

2 and 3. "Were these [sixteen varieties of eggs] seen to be deposited by the bird, or how were they identified as those of the cuckow? . . . . Is there not room for error here?"

The evidence on which the eggs in question were referred to the Cuckow has been printed in full by Dr. Baldamus and the translator of his essay. To repeat it here would occupy much space and, I think, be unnecessary. It is of much the same kind as the evidence with regard to most Cuckows' eggs. I will freely grant that it might be more satisfactory—if it were so my former paper would never have been written, since naturalists must then have at once accepted the theory. But, on the other hand, I have a right to ask this: If the eggs in question were not Cuckows', what birds laid them? Surely not those in whose nests they were found, because it is a fact which most oologists will confirm, that when birds lay larger eggs than usual the colouring is commonly less deep, and though exceptions may occasionally be found, yet here we have sixteen which are at the same time larger than usual, and of a colour at least as deep, supposing them to belong to the nest-owners. *Sixteen* cases are too many to be exceptional, but this is the number only of the specimens *figured* by Dr. Baldamus; upwards of *sixty* are more or less fully *described* by him.

4. "How then is this process effected?"

In answer to this, Mr. Sterland quotes a very brief summary of my own explanation, to which I have nothing now to add.

5 to 9. The next five questions, for brevity's sake, I will not repeat. They are very pertinent, but are far more easily asked than answered, for they open a wide field of speculation and investigation, since all the hitherto unexplained phenomena of "Dimorphism," "Trimorphism," and "Polymorphism," seem to enter here. But with respect to one of the questions (No. 6), I submit that even if there were no other instance satisfying the conditions imposed by Mr. Sterland than that which I alleged, it is no true argument against the truth of what I advanced. But I think there is an indication of it in other species bearing very directly on the point. Take the Blackcap Warbler and the Tree-Pipit. The eggs of the first are well known to present at least two very different appearances, and those of the second are still more variable. Since Mr. Sterland will not allow that my Eagles fulfil his conditions (and of course he has a perfect right to do so), perhaps he will permit me to bring forward these birds. I have some reason for believing that the same hen Blackcap constantly lays eggs of similar colour. Do the birds of this species hatched from eggs with reddish shells lay eggs of the same character, or brownish ones, and *vice versâ?* If of the same character, we have such an example as is required. If of the other colour, it becomes a case in some measure of "Alternate Generation," but still reducible to a law. That there should be no law at all seems to me at least unlikely, though I fear its discovery is hard.

Certain facts of Dimorphism and Polymorphism are known, but I have not met with any attempted explanation of the phenomena even in such decided and remarkable cases as those of the Malayan Butterflies given by Mr. Wallace (Trans. Linn. Soc. vol. xxv. pp. 5-11). Why the different forms of one species of *Papilio* inhabiting the same district remain distinct is perhaps more unaccountable than that the different forms of Cuckows' eggs should be preserved, for it does not seem to me unlikely that the colour of the egg and the maternal instincts should depend upon the *hen* bird; in which case, granting the hereditariness (if I may make such a word) of the qualities already specified, I think there would be no difficulty.

10. A full reply to Mr. Sterland's last question would lead me to anticipate much that I intend to say when you again permit me to trespass on your readers' forbearance. Consequently, I must defer it until I come to the consideration of "Cuckows' Dupes." ALFRED NEWTON

Cambridge, Dec. 11, 1869

By way of postscript of my letter of the 11th of December (for the delay in publication of which I am in no way accountable*), permit me to offer a few remarks on the communications of Mr. Dresser and Mr. Cecil Smith which have since appeared.

Mr. Dresser says (p. 218) that he "cannot quite agree with Professor Newton that Cuckows' eggs *as a rule* are subject to great variety." I am not aware that I had made such an assertion. The nearest approach to it that I can find is my statement (p. 74), that "it has long been notorious to oologists that eggs of the Cuckow (*i.e.* of the Common Cuckow of Europe—the only species I had mentioned) are subject to very great variety," and in proof thereof I have since furnished some other (and, I think, satisfactory) evidence. Mr. Dresser himself has also brought two or three additional examples which confirm my statement. For the knowledge of these I am much obliged to him, as well as for stating the result of his own experience in support of my supposition that the eggs of the same hen Cuckow resemble each other.

Mr. Cecil Smith (p. 242) seems to me to be as unfortunate in his interpretation of my remarks as he was in that of Dr. Baldamus's (p. 75, note). I feel sure that I have not "pruned and paved" down the doctor's theory so "that there is but little of the original left." To the facts alleged by that naturalist I have taken no exception—on the contrary, I have borne witness (pp. 74, 75) to their general truth; and in the attempt to offer a reasonable explanation of them, I am certain that my "manipulation" is not open to any such charge as that made by Mr. Smith. My "cautious and limited statement" is not different from that of the doctor, nor does "it entirely sweep away" a single assertion of his as to matters of fact. Mr. Smith, apparently, thinks because I have referred to the number of Cuckows' eggs yearly found in nests of the Hedge-Sparrow in this country, without ever bearing any resemblance to the eggs of that bird—a fact, of course, fully admitted by him—that I must thereby deny the single exceptional case adduced from Germany by Dr. Baldamus; but I have never maintained, because no likeness is to be traced in a great many instances, that none was ever perceptible, and accordingly there is no "issue of fact" between the doctor and myself. I must take the liberty of adding, that Mr. Smith, having, as I before showed, misunderstood Dr. Baldamus, has now misunderstood me; and this being the case, it is perhaps needless for me to take up more of your space. ALFRED NEWTON

January 3, 1870

The Veined Structure of Glaciers

I THINK there is no one point in connection with glaciers more interesting than their veined structure, or one upon which so much has been written that remains equally unsettled. The difference of opinion about it between the authors who have published most upon the subject are not less remarkable than the phenomenon itself: no two are agreed, except in considering it as a constitutional feature.

Professor Agassiz maintains (*Atlantic Monthly*, Dec. 1863) that the horizontal layers of pure ice which are formed between the beds of snow from which a glacier is born, constitute many of the identical veins or plates of pure ice which pervade the glacier when it is in full life and activity; and attributes the inclination which they make, in the latter case, to their former horizontal position, to the contortion, bending, or folding, to

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* They are Naumann, Thienemann, Brehm, Gloger, and Von Homeyer. Unfortunately, Dr. Baldamus does not refer to the passages in their writings wherein this opinion is expressed; and as most of these writings are somewhat voluminous, I have not always been able to find what are the passages meant. I presume that Mr. Sterland has been more fortunate, for he would scarcely doubt the assertions without knowing what they were, and I should be much indebted to him if he will tell me where they occur—indeed, I am uncertain which of the Brehms and which of the Von Homeyers is intended.

* [The delay in the publication of Prof. Newton's letter is owing to an oversight. It was received prior to the communications of Mr. Dresser and Mr. Cecil Smith, printed in our eighth and ninth numbers.—ED.]

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